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Abstract
We assess the impact of lowering the cost of learning to drive in France by randomly assigning candidates to either of two groups of 18 to 25 years olds. Young people in the "test group" were given a €1000 voucher to pay for their driving lessons and were suported by a welfare centre throughout the time they were learning. Young people in the "control group" retained all the other welfare benefits for the underprivileged. The vouchers were given to 10 000 young people most of whom were not in education, employment or training. We investigate three types of outcome covering driving, housing and employment status. We analyse the specific role of local support in passing the driving test and we specifically take into account the possibility of spillover effects between treated and untreated individuals.

Keywords: Randomised Controlled Trials, NEET, driving licence

JEL: H22, J64, L38

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1. Introduction

It is widely reported in the literature that the most vulnerable social groups are the ones most heavily penalised by the high cost of public and private transport. Social inclusion might therefore be improved by reducing the cost of transport. Better public transport networks can have a positive effect on employment for people from isolated and underprivileged neighbourhoods (Holzer *et al.*, 2003). Having the use of a car can increase both the chances of being employed and the quality of the job measured by the number of hours worked or the pay level (Raphael and Rice, 2002; Gurley and Bruce, 2005). This effect is particularly marked for minimum income recipients who are potentially the people furthest from employment (Ong 2002; Blumenberg and Hess, 2003). Conversely, lack of access to public transport and to private vehicles deteriorates the situation on the labour market, especially for those living in the most deprived neighbourhoods (Kawabata, 2003; Ong and Miller, 2005).

Accordingly, social arguments are very often put forward to support targeted accessibility policies in the United States (Cervero *et al.*, 2002.), the UK (Church, 2000; Lucas, 2006; Preston and Raje, 2007), France (Bertrand, 2005; Fol *et al.*, 2007; Avrillier *et al.*, 2010) and Spain (Cebollada, 2009), among others countries. One of the most widely recommended actions is to reduce the cost of passing the driving test for the most vulnerable social groups. By assessing the linkages between expensive driving instruction and its substitution elasticity with public transport, Priya and Uteng (2009) show that when the cost of driving lessons is prohibitive for certain groups, improved public transport networks may surprisingly lead to even further social exclusion.

This problem is particularly acute in France where the cost of driving lessons is high, as in the UK and northern European countries. By contrast, most US states make it fast and simple to learn to drive and there are also graduated systems to phase in full privilege driving in some countries, such as Australia, New Zealand and Canada. In France, learning to drive is a long and difficult process for young people and driving licences have been made probationary for all new drivers since 2004 (new drivers begin with an initial capital of 6 points and are only issued the standard 12 points after three years without having any points deducted for offences). To get a driving licence in France, candidates first have to pass a theoretical test and then a practical driving test. This involves expensive instruction possibly lasting more than a year and a minimum outlay of about €1500. The pass rate is lower than 60%.

This very high cost coincides with a high rate of unemployment in France, especially among

young unskilled people. This may explain the wide range of aid packages from central and local government aimed at the young and unemployed to cut the cost of learning to drive. It seems that in France great benefit is expected from these mechanisms.

However, is it true that lowering the cost of learning to drive leads to better social inclusion? What are the real effects of cutting the cost of learning to drive for the least privileged social groups? Is it really necessary to maintain or introduce subsidies to reduce the cost of learning to drive?

In this paper, we assess the effects of reducing the cost of learning to drive in France by randomly assigning candidates to one of two groups of young people aged between 18 and 25 years. The "test group" were given a €1000 voucher to pay for their driving lessons and were suported by a welfare centre throughout their driving instruction. The "control group" retained all the other welfare benefits for the underprivileged. We evaluate the impact of the voucher mechanism by comparing the test and control groups through two surveys conducted one and two years after respondents joined the experiment, set against contextual and local data. The vouchers were given to 10 000 young people most of whom were not in education, employment or training (NEET). We suspect *a priori* that for young people in this group, passing the driving test will have a big impact in terms of social integration and finding work.

The experiment was entitled "*Dix mile permis pour réussir*" (Ten thousand licences for success) and it was launched in 2009 by the *Fonds d'Expérimentation pour la Jeunesse*.[1] Our evaluation covers a sub-sample of 154 non-profit organisations throughout France, mostly "*missions locales*" that supported a total of nearly 6000 young people by providing financial assistance and helping them to take their driving tests between January 2010 and June 2012. We assess the impact of this treatment on three types of outcome covering driving, housing and employment status. Furthermore, we investigate the specific effect of local support in passing the driving test and we specifically take into account the possibility of spillover effects between treated and untreated individuals, using the method set out by Baird *et al.* (2014) based on randomly varying saturation rates.

The present paper is in line with the growing literature reporting randomised experiments to evaluate public policies (Miguel and Kremer, 2004; Banerjee and Duflo, 2009). Although

[1] Created in late 2008, the *Fonds d'Expérimentation pour la Jeunesse* (FEJ) is an innovation in the French institutional landscape. It combines contributions from the state and private partners to fund experimental mechanisms for young people. Initially focused on social and occupational integration, the fund was incorporated into the Ministry of Education in 2010 and currently covers all school policies that affect all young people aged under 25 years.

such experiments have recently been developed in France for job finding (e.g. Crépon *et al*., 2013), they are still comparatively rare, especially on so a large scale as the "*Dix mille permis pour réussir*". This large scale encompasses a wide variety of local economic and social contexts, which is a major source of external validity. To the best of our knowledge, the only paper that has previously taken an experimental approach in respect of driving licences is Bertrand *et al.* (2007), which analysed corruption in India by monitoring 822 driving test candidates.

The paper proceeds as follows. Section 2 describes the process for obtaining a driving licence in France and the welfare benefits for candidates from underprivileged groups. Section 3 sets out the design of the experiment and describes how data were collected. Experimental findings and robustness checks are given in section 4. Section 5 concludes.

2. Institutional setting

With 1.7 million tests and 1.02 million passes per year, the driving test is the leading examination in France, and for candidates it is one of the most expensive and most difficult. Passing the test is especially important for young people with integration difficulties. To obtain a driving licence, candidates must pass both a theory test and a practical test. The process is lengthy, costly and risky with a final pass rate of 60%. We explain how the driving test is organised in France before describing the aid mechanisms targeted at the least privileged social groups.

The driving licence in France

Obtaining a driving licence (category B) involves passing two tests, a theory test and a practical driving test. To pass the theoretical test, candidates must answer a series of 40 multiple choice questions with fewer than five errors within a time limit. After enrolling at a driving school, it takes four months before being able to take the theory test. Candidates who fail must wait at least two weeks before they can re-sit the test. After passing the theory test, candidates must wait two weeks before taking the practical test. Candidates who do not pass the practical driving test within three years and/or five attempts must take the theoretical test again.

The practical test involves driving with a licensed examiner for about 30 minutes. It measures compliance with the highway code, knowledge of the vehicle and ability to detect major

technical faults, mastery of the controls and handling of the vehicle to avoid creating dangerous situations, ability to ensure the driver's own safety and that of other users on any type of road, ability to perceive and anticipate hazards arising from movement and act appropriately, level of autonomy in making a journey, ability to drive respecting the environment and behave in a civil and considerate way to other road users, especially the most vulnerable (Source: Article 1 of the Decree of 19 February 2010 on the procedure for the practical test of the examination for the category B driving licence).

Failing the practical test entails waiting for a minimum of two weeks before the candidate can register again for an upcoming session. The lack of availability of examiners means some candidates may have to wait several months before they can take the practical test again and may have to take (costly) additional lessons. Since the driving test reform of 13 January 2009, the practical test has been based on a skills assessment instead of an error report. The pass rate exceeded 60% (60.25%) for the first time in July 2010. It was 56.6% in late 2008.

Welfare benefits for learning to drive

There are many welfare programmes designed to lower the cost of driving lessons. Central government provides a mechanism for zero-interest loans of €800 to €1200 called "*le permis à un euro par jour*" (your licence for a euro a day) and contributes to other mechanisms through the Youth Trust Fund. Job centres offer job seekers support of up to €1200 plus €1500 for people on minimum income allowance (RSA[2]) using the personalised welfare-to-work aid (APRE[3]) to finance all or part of the cost of learning to drive. There are also regional council programmes to reduce the cost of learning to drive in 13 of France's 22 regions. These take the form of regional tax exemption, financial assistance and grants for young and unskilled people. There are also 33 aid packages offered by area councils (*départements*) in the form of grants for learning to drive and targeted support for the young and RSA recipients. Finally, 111 city councils (including Ajaccio, Cambrai, Massy-Palaiseau, Suresnes) also provide aid packages, which are usually means-tested, and work schemes. Among these localities, 84 municipal council and five joint municipal councils have adopted a system of grants for learning to drive, combined with the €1 per day scheme, allowing young people to work for a local authority in exchange for funding to learn to drive. There is a standard practice of providing support for learning to drive including a wide range of mechanisms funded by all tiers of government.

[2] RSA: *Revenu de Solidarité Active*.
[3] APRE: *Aide Personnalisée au Retour à l'Emploi*.

3. Empirical strategy

We conducted a controlled experiment to assess the effect of the financial subsidy. It is described in this section (31.), together with the main outcome of the randomisation (32.) and the way the data required for the econometric analysis were collected (33.).

31. Experimental design

The "*Dix mille permis pour réussir*" experiment was launched by the *Fonds d'Expérimentation pour la Jeunesse* during summer 2009 by way of an call for tenders aimed at all organisations in contact with young people with insertion difficulties in order to support them in learing to drive. In all, 58 organisations signed up to the scheme. They were groups of *missions locales*,[4] (neighbourhood offices) which are non-profit youth support centres, local government authorities in contact with *missions locales* and non-profit driving schools. Each project involved a varying number of young people, from a few dozen to several hundreds. For the evaluation, it was decided to distinguish between 28 small non-profit organisations involving fewer then 100 young people, and 30 large non-profit organisations involving at least 100 young people. The quantitative evaluation covered only the 30 large non-profit organisations that grouped 154 different centres. These centres, essentially *missions locales*, are dispersed throughout France and are located in various socio-economic contexts (from rural to urban areas). This broad geographical coverage and the variety of local economic and social contexts are particularly attractive features of this large controlled experiment and greatly reinforce its external validity. Each centre supported from 8 to 150 young people. In total, 6000 young people were supported by the 30 large non-profit organisations and were included in the evaluation protocol.

[Figure 1 about here]

The experiment lasted two years (Figure 2). Young people began joining the experiment in January 2010. They were proposed individually by their case worker, often a social worker in a *mission locale,* by completing a single form using an extranet site specifically designed for

[4] Neighbourhood offices for the social and professional integration of young people (commonly referred to as *missions locales*) are, in France, in charge of informing and guiding youth toward occupational and social integration. Created in 1982, they form a network of more than 450 centres covering the entire national territory. These non-profit organisations are usually chaired by the mayor.

the experiment. All the potential beneficiaries completed this form that captured their social and occupational status. The forms were centralised and statistically processed to describe the candidates' profiles and eliminate the most atypical applications.

[Figure 2 about here]

Young people were assigned to either the control or test group based on a random draw. This random choice was made as and when candidates joined the experiment over an eight-month period from January 2010 to September 2010 based on the extranet files. The evaluators gave the local centres the result of the random draw a week later: three out of four young people were awarded the subsidy. The one in four non-beneficiaries formed the control group and had access to the other aids available locally. If a young person was assigned to the control group, the local centres undertook not to try to compensate for that status by awarding exceptional support not available to other young people not in the experiment. Exceptionally, when an experimenter thought it necessary for a specific individual to join the programme and benefit from the subsidy (because of their personal history, situation or project), the person joined the programme but was not included in the random draw. This possibility, the 'joker', was strictly limited and grounds had to be stated. Experimenters had at most one joker for every 30 applications.

At the end of September 2010, there were in total 8121 validated application files in the experiment. Of those files, 7143 went into the random draw, with 5350 (75%) in the test group and 1793 (25%) in the control group. The files omited from the experiment were those of the small organisations (943) and the jokers (35) that came to just 0.4% of the total.

All the non-profit organisations joined in the action plan progressively. The large organisations entered more rapidly and were all effective by June 2010. Half of the young people were already in the experiment by May 2010. Some young people joined after September 2010 but they were omitted from the statistical analysis. The first survey was conducted between April and May 2011, the second survey at the same period 12 months later. Each survey covered 1800 young people and no respondent was surveyed twice. Respondents were randomly selected from the test and control groups. A separate survey of the 154 local sites in contact with the young people was conducted early in 2012 to collect additional information about the local context, other financial aid available to the young people and the kind of non-monetary support in place. Because the other financial aids available varied considerably from one place to another, some young people in the control

group had access to financial aids other than the €1000 subsidy. However, these aids were not as high as that given in the experiment. We controlled for these differences in the evaluation by adding control variables on the level of resources.

When they enrolled in the experiment, all the young people were informed there was no guarantee they would receive the subsidy. However, all were asked to respond to the telephone surveys 12 and 24 months after enrolling. Since all the young people enrolled voluntarily, the Intention to Treat Effect (ITT) in our case was equal to the Average Treatment Effect (ATE). The overal protocol of the experiment is displayed in Figure 3.

[Figure 3 about here]

In the end, for the first survey, 828 individuals were in the control group and 1000 individuals were in the test group. For the second survey, 629 individuals were in control group and 1270 in the test group.

32. Outcome of the randomisation

To check that our random assignment was successful in balancing the control and test groups, we compared the groups with respect to pre-treatment observable characteristics for both surveys (Table 1). We found that the test and control groups were similar in terms of gender, non-French citizenship, proportion of young people with a compulsory education level, work experience, driving experience, possession of a moped, ASR or BSR qualification,[5] resources, occupational status and type of housing. However, the difference was significant for age, although the mean age was very similar in both groups (22.9 vs. 22.76 years). It appears that for upper secondary education, the proportion was slightly higher in the test group (0.169 vs 0.138) but the difference was no longer significant when broken down by different educational categories (general, technological, vocational). Conversely, the proportion of university-educated young people was slightly lower in the test group (0.016 vs 0.030). However, the frequency of that category was minimal (fewer than 20 individuals) so that the two groups had similar educational attainments. Finally, we found that individuals

[5] The certificate of road safety (*Attestation de Sécurité Routière*, ASR) is usually awarded at school and consists of two levels validated after theoretical tests (ASSR1 and ASSR2). Obtaining Level 1 gives access to practical training in driving school, which provides another certificate that is required to ride a moped (*Brevet de Sécurité Routière*, BSR).

having a CIVIS contract[6] were slightly more numerous in the test group (0.646 vs 0.673) but the difference was only significant at the 10% level.

Globally, the randomisation therefore produced a good balance between the groups. The young people in the experiment were 22 years old on average, with a low educational attainment, no work experience, little if any theoretical and practical knowledge of driving, still living with their parents and mostly unemployed.

[Table 1 about here]

33. Outcomes and other data

The surveys provided a set of information from which we constructed outcomes pertaining to three main categories:

- "driving outcomes": passing the theory test, passing the practical test, car ownership.
- "housing outcomes": living in independent housing, moving within the last 12 months
- "employment outcomes": being an employee, having a permanent job, having a temporary job.

We selected these three categories in order to measure the effects of the subsidy on the social integration of young people and their self-reliance in life. This can be broken down into own means of transport, independent housing and financial independence, which is linked to access to employment. While our experimental design enabled us to measure the causal effect of the subsidy on vehicle use, this was not the case for access to employment and housing. Indeed, for employment and housing there are self-selection processes that we cannot fully control for and that may result in specific bias. We nevertheless included them in the analysis as they provide interesting insights into the effects of the subsidy.

In the evaluation, the pre-treatment characteristics were used to construct control variables for each outcome. We also combined other sources of administrative data to construct contextual variables specific to each category of outcome (Table 2). These variables were measured at the lowest local government level (*commune*) where the young person lived and aimed at capturing the transport service (driving outcomes), tensions on the local housing market (housing outcomes) and tensions on the labor market (employment outcomes). Moreover, for

[6] The CIVIS contract is a plan of action to help work insertion for young people with difficulties. When they subscribe to such a contract, a case worker supports them in their career path. These contracts are concluded in *missions locales* or in information and careers guidance centres.

all outcomes, we included population density and its square to include possible non-linear effects of that dimension. The definition and source of these contextual variables are displayed in Table 2.

[Table 2 about here]

4. Results

The effects of the €1000 subsidy on several outcomes are investigated using the following model:

$$Y_i = \beta_0 + \beta_1 T_i + x_i'\theta + \varepsilon_i \quad (1)$$

where Y_i is the outcome of individual i, T_i is the indicator of being treated by receiving the €1000 subsidy and ε_i is the error term. To ensure balance and residual variance, we also include a vector of control characteristics x_i. This vector includes pre-treatment characteristics for each individual (see section 31.) and some specific contextual variables for each category of outcome as described in Table 2. Equation (1) is estimated by Ordinary Least Squares (OLS) for each outcome, for both surveys, with and without control variables and with standard errors clustered at the local site level. The results for each category of outcome, "driving", "housing" and "employment", are presented in sections 41., 42. and 43. respectively. In section 44. we investigate the possibility of spillovers from the treated to the non-treated individuals. Finally, in section 45. we study the effects of the selection procedure and the support provided by the local sites on a restricted sample of individuals.

41. Driving outcomes

The estimation results for the three "driving" outcomes (passing the theory test, passing the practical test and owning a car) are displayed in Table 3. Four estimation results are presented for each outcome: two surveys, with and without control variables. For the theory test outcome, the sample was reduced as some individuals had already passed it before the experiment began. They were therefore removed from the sample.

The €1000 subsidy was clearly beneficial at each step in the process of learning to drive as the coefficient pertaining to treatment is positive and significant for all three "driving" outcomes.

For instance, the difference between treated and untreated individuals is about 0.2 for passing the theory test, a figure that is a stable for both surveys. However, the effect is higher for the second survey for passing the practical test (around 0.11 for the first survey and 0.14 for the second) and much higher for owning a car (0.06 for the first survey and 0.15 for the second). These figures can be applied to the whole experiment to provide an order of magnitude of the effects. Two years after the subsidy was granted to 10 000 individuals, without help about 2100 of them would not have passed the theory test, about 1400 would not have passed the practical test and 1400 would not have owned their own car. While the subsidy and the non-monetary support have important positive effects on passing the theory test, practical test and car ownership, the chances remain low in absolute terms, even more so if we consider that all the young people volunteered to participate in the experiment. Two years after it began, one young person in two failed the theory test, one in two failed the practical test and one in three did not have a car. These figures illustrate the major difficulties for young people with insertion problems in gaining access to vehicle use in France.

Turning to the individuals' pre-treatment characteristics, it is easier for younger, more educated individuals and moped owners to pass the theory test, the practical test and to own a car. Having more than five hours previous driving experience also positively impacts the outcome while previous work experience has a strong positive effect but only in the first year. Some discrimination against non-French citizens is also apparent. Conversely, employment status and type of housing make no difference. With respect to environmental characteristics, population density has a marked non-linear effect on outcomes.

[Table 3 around here]

In order to understand these results, we computed the conditional probabilities of success, i.e. the probability of moving on to the next stage of the journey to vehicle use for those who passed the previous stage. The results are displayed in Table 4. The proportion of young people who passed the theory test at the first, second or third attempts is the same in both groups for both surveys. Hence, the subsidy and the support do not increase the chances of passing the theory test. The reason the young people in the test group have a higher success rate in the theory test is that they could take the test more times.

Similarly, for the young people who passed the theory test, being treated markedly increased the chances of passing the practical test. Indeed, the pass rate for the practical test is 47.6% for treated young people versus 40.1% for the others, with a difference that is significant for

the first survey (but not the second). However, the proportion of young people that passed the practical test at the first attempt is similar in both groups for both surveys. Hence, subsidy and support do not increase the pass rate for the practical test but they do mean candidates could take the test more times. This is confirmed by the fact that the treated young people did have more driving lessons: 32.2 hours 24 months after the beginning of the experiment compared to 24.6 for those in the control group.

[Table 4 around here]

42. Housing outcomes

The estimation results for the two "housing" outcomes (living in independent housing, having moved during the last 12 months) are displayed in Table 5. Interestingly, while the effect of a subsidy is significantly positive one year after it was given, it turns out to be slightly negative after two years. With respect to moving, the effect is significant and negative for both years, although higher in absolute value the first year. This may highlight a lock-in effect of the programme: young people must stay at the same local site throughout the period of instruction in order to benefit from the subvention and benefit from all the non-monetary support. This limits their residential mobility. The effects of control variables are quite similar for the two outcomes. Women are more prone to be in independent housing for both years and not surprisingly, individuals who were not living with their parents at the time of the programme are more prone to be in independent housing both 1 and 2 years after the programme or to move. Individuals with work experience display higher mobility as well. Finally, population density is significantly negative, highlighting a congestion effect in the housing market in high-density areas.

[Table 5 around here]

43. Employment outcomes

The estimation results for the three employment outcomes (being an employee, having a permanent job, having a temporary job) are displayed in Table 6. Globally, the effects of the subsidy on employment are very limited[7]. In particular, while treated individuals are not more

[7] Note that we computed for each case the Minimum Detectable Effect (MDE). For the employment outcomes, the MDE is around to 0.117 for the model without covariates and 0.115 for the model with covariates when the power is set to 80% and a significance level of 5%. For a power of 50%, the MDE is around 0.076. Hence, the absence of

likely to be wage labourers or to have permanent jobs 1 or 2 years after the programme, the impact of the subsidy appears to be slightly positive and significant 2 years after the programme for temporary jobs. Conversely, education is positive and strongly significant for the first and third outcomes reflecting the importance of this factor in finding work. High inertia also appears as individuals already in work during the programme more often had temporary or permanent jobs. Again, the congestion effect is at work as highlighted by the negative sign of population density.

[Table 6 around here]

44. Spillovers

In this section, we specifically take into account the possibility of spillover effects between treated and untreated individuals. Indeed, as both treated and untreated young people were monitored by the same local site, they were able to communicate and exchange information. Hence, in order to properly assess the programme's impact, we need to control for these exchanges and the possibility of spillovers between treated and non-treated individuals. Interference between individuals has long been seen as the Achilles heel of controlled experiments because standard experimental designs are unable to identify and measure spillovers while the statistical techniques aimed at evaluating programme impacts are overwhelmingly based on the Stable Unit Treatment Value Assumption (SUTVA). Given this concern, empirical work has emerged in the past decade attempting to relax this assumption that individuals are unaffected by the treatment status of others. Some authors identify network effects using experimental variation across treatment groups (Miguel and Kremer, 2004; Bobba and Gignoux, 2013), some exploit plausibly exogenous variation in within network treatments (Babcock and Hartman, 2010) or control for spatial spillovers by creating distance bands around each treated individual (Bobba and Gignoux, 2013).

In this study, we use the method set out by Baird *et al*. (2014) to control for possible spillover effects among treated and untreated young people. These authors argue that a randomised saturation design, which is a design where there is an experimental variation of the rate of treated individuals between clusters, reveals spillovers. In our case, the clusters are the local sites. They also provide a method by which to estimate the counterfactual situation when no pure control outcome is available. We apply this in our case since the programme design does

significance of the treatment may also be due to low power of the test for these outcomes. At any rate, these results suggest that the effect of the treatment is at best very low.

not incorporate pure control clusters, i.e. clusters where the saturation rate is zero.

We computed the saturation rate for each local site by dividing the total number of young people actually helped by the total number of eligible young people locally. The latter was calculated from the number of young adults in each locality from census data, multiplied by the national proportion of driving licence holders among all young people and by the proportion of young people with integration problems among all young people. The density of their distribution over the local site is displayed in Figure 4. There is a main mode around a saturation of 10% and a secondary mode around a saturation of 45%. The distribution is highly skewed to the right. Nearly two-thirds of the sites meet less than 50 young people and are thus characterised by often weak saturation rates. Among the sites that concentrate many young people in the experiment, some are located in rural areas or small towns leading to high levels of saturation (e.g. Bondy, Seine-Saint-Denis with 131 young people and a saturation rate of 67%, La Seyne, in the Var, with 135 young people and a saturation rate of 64% and Perpignan with 233 young people and an estimated 74% saturation rate). Other sites that attracted just as many young people are located in densely populated areas leading to low saturation rates (as in Venissieux, south of Lyon, where the saturation level is only 9% for 154 young people or Saint-Etienne where it is 8% with 139 young people). In all, it seems reasonable to assume that the saturation distribution is random.

Formally, in the absence of a pure control cluster Baird *et al.* (2014) suggest estimating the following equation:

$$Y_{im} = \alpha_0 + \alpha_1 T_{ic} + \alpha_2 \pi_m + \alpha_2 (T_{ic} * \pi_c) + x'_{im}\theta + \varepsilon_{ic} \quad (2)$$

where the additional subscript *m* indicates the m^{th} local site and π_c is the saturation rate of the m^{th} local site. In the random saturation design with no pure control, the hypothesis test $\alpha_2 = 0$ indicates whether there is a variation in the control outcome across saturations. If this hypothesis is rejected, then spillovers are present on the untreated individuals and it is necessary to correct the counterfactual. In this case, the unbiased estimate of the Intention To Treat (ITT) is equal to $\hat{\beta}_1 + \hat{\beta}_0 - \hat{\alpha}_0$ where $\hat{\beta}_0$ and $\hat{\beta}_1$ are the estimated coefficients in equation (1) estimated by weighted least squares, using the saturation rates and weights. The spillovers on the non-treated can be estimated as $\hat{\beta}_0 - \hat{\alpha}_0$.

Tables 7, 8 and 9 show the OLS estimation results of equation (2) for all the outcomes and

standard errors clustered at the local site level. To save space, the estimations for the control variables have been omitted.

For the driving outcomes, the coefficient α_2 is never significant, so spillovers on the treated subjects are not detected. However, for the second survey, the interaction coefficient between saturation rate and treatment is significant and negative, which could highlight a congestion effect and delays for young people to be able to take the driving test. Remember it is necessary for an examiner to be available for the practical driving test. However, the number of examiners is limited locally, which may explain a negative spillover on untreated young people.

For the housing outcomes the coefficient α_2 is only significant at the 10% level for the first survey and the first outcome (living in independent housing). Hence in that case, we reestimated equation (1) with weighted least squares[8] to compute the unbiased intention to treat. It is -0.1167 corresponding to a negative spillover on the treated effect of 0.2467. The lock-in effect detected for the second survey seems then to be already present for the first survey.

Finally, for the employement outcomes, the coefficient α_2 is never significant. However, as for the driving test, we observe that for survey 2, the interaction effect between saturation and treatment is significant at 10% and negative for the first two outcomes (being an employee and having permanent job) for the second survey. Again, this could highlight some congestion effect on the local job market.

[Tables 7, 8 and 9 around here]

45. The role of local support

We investigate here the specific role of the local non-profit organisation in supporting young people in learning to drive. For that purpose, a telephone survey was conducted after the experiment to determine how each local site selected the young people to participate in the experiment and the kind of support they provided. Unfortunately, some local sites were reluctant to respond to the survey. Of the 147 local sites that participated in the experiment, 47 did not answer. We are aware, then, that the results presented in this section could be affected by selection bias and should be interpreted with caution.

In order to summarise the responses from the local sites, we conducted a hierarchical

[8] The estimation results are available from the authors upon request.

clustering analysis based on Ward's criterion. This procedure allowed us to construct four groups of local sites for the type of selection and four groups of local sites for the type of support provided.[9]

In the first case, we distinguished between local sites that (i) selected young people based on occupational criteria (n = 28), (ii) selected young people based on social/financial criteria (n = 25), (iii) selected young people based on territorial criteria to ensure a homogenous coverage of their area of intervention (n = 19) and (iv) that made the selection internally in a commission, without predefined criteria (n = 23). We then constructed three dummies for the first three categories and took the first category as the reference.

In the second case, we distinguished between local sites that (i) only provided collective support for the theory test (n = 10), (ii) provided light support for the theory and practical tests with some information sessions (n = 24), (iii) provided intensive support for both theory and practical tests and organised information sessions on road safety and first aid (n = 16) and (iv) provided individual support with regular meetings with a case worker but without instruction (n = 49). We constructed three dummies corresponding to the first three categories and took the last category as the reference.

The estimation results for all outcomes are displayed in Tables 10, 11 and 12. For the "driving" outcomes, the treatment estimates are qualitatively similar, with all coefficients significant and positive. However, with respect to the complete sample, they are slightly lower, falling, for instance, from 0.21 to 0.19 for passing the theory test the first year. None of the selection dummies are significant except for individuals selected on the basis of social criteria that were slightly more successful on the theory test for the first year. The associated coefficient is only significant at 10%, though. With respect to the type of support, the young people receiving complete intensive collective support seemed to be less successful at the practical driving test the second year than the young people who received less intensive or individual support. The same results apply for the young people having moved within the last 12 months and with temporary jobs. It therefore seems that individual meetings are more effective for helping young people than collective meetings, even when they are intensive.

[Tables 10, 11 and 12 around here]

[9] Detailed explanations on how the groups were formed are available from the authors upon request.

5. Conclusion

Funding coupled with a support system for learning to drive focused on young underprivileged people increases access to driving schools, pass rates for the theory test, pass rates for the practical test and vehicle ownership. The effect is positive on the mobility of young people. However, two years after joining the programme, one in three failed the theory test, more than one in two failed the practical test and nearly two in three did not own cars. With a budget of €10 million given to 10 000 young people, 1400 driving licenses were issued that would not have been without the aid. This represents a cost of around €7150 per licence, which is more or less five times the typical cost of learning to drive. This high cost argues against generalising this type of funding. More generally, it suggests that the funding systems for learning to drive operated by central and local government that are less intensive and less generous than the "*Dix mille permis pour réussir*" programme probably have even more modest effects. However, we should not conclude that the experiment has been a failure. If the mechanism had been generalised without having been experimented beforehand, the cost for public finances would have been substantial. To fund learning to drive for 150,000 young French people without qualifications would have required a budget of €1 billion, which seems prohibitive. All in all, these figures summarise the huge difficulty for NEET young people to learn to drive in France.

Learning to drive is intensive, selective and long-term like vocational training, limiting further investment in the occupational sphere as well as in the non-occupational sphere. From analysis of the data from two statistical surveys of young people 12 and 24 months after joining the experiment, the results seem broadly dominated by short-term effects of lock-in. When learning to drive, young people are less geographically mobile, less actively seeking jobs or training courses, or a better job than the one they have. Once they have passed the test, these characteristics are reversed. Young people gain in residential mobility and find work more easily.

Financial support awarded to young people to learn to drive therefore has positive long-term effects, but the effects are negative in the short run. Given the magnitude of the lock-in effects, it is necessary to make it easier for young people to learn to drive in order to reduce the financial cost but also the length and difficulty of the process. Simplifying the driving test on the model of many other countries would produce a double dividend in increasing the mobility of young people and making it easier for them to find employment.

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Tables and figures

Table 1. Results of the experiment

	Test group	Control group	Difference	t test
Women	0.561	0.573	-0.012	-0.703
Age	22.944	22.759	0.185	2.529**
Foreign	0.093	0.103	-0.009	-0.951
Educ - compulsory	0.484	0.499	-0.015	-0.889
Education - upper sec	0.169	0.138	0.031	2.566**
Education - university	0.016	0.030	-0.013	-2.564**
Work experience	0.790	0.807	-0.017	-1.294
No driving experience	0.649	0.667	-0.019	-1.203
<5hrs driving experience	0.149	0.147	0.002	0.143
>5hrs driving experience	0.202	0.184	0.017	1.319
Moped	0.120	0.105	0.015	1.383
ASR or BSR	0.201	0.207	-0.007	-0.487
CIVIS	0.646	0.673	-0.027	-1.692*
Resources	304.084	286.231	17.854	1.554
In job training	0.123	0.123	-0.000	-0.0594
Unemployed	0.725	0.748	-0.023	-1.588
Employed	0.152	0.128	0.024	2.081
Living with parents	0.655	0.633	0.022	1.359
Independent housing	0.236	0.248	-0.012	-0.859
Other housing	0.109	0.119	-0.009	-0.888

Notes: Column 1 displays mean characteristics for the test group and column 2 for the control group.
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 2. Contextual variables; communal level

	Definition	Source
Population density	Ratio of population over communal area in 2011	General census (2011)
Road density	Ratio of kilometres of road over departmental area in 2011	General census (2011)
Railway density	Ratio of kilometres of railway over departmental area in 2011	General census (2011)
% Vacant housing	Communal rate of vacant housing in 2010	INSEE (2010)
New households	Proportion of households having moved in the communes in 2 years before 2010	INSEE (2010)
% Unemployment	Unemployment rate in 2010	INSEE (2010)
Active/employed	Ratio of active population to total number of jobs in commune in 2010	INSEE (2010)
Active/establishment	Ratio of active population to total number of establishments in commune in 2010	INSEE (2010)

Table 3. Estimation results for driving outcomes

	Theory test				Practical test				Car ownership			
	Survey 1		Survey 2		Survey 1		Survey 2		Survey 1		Survey 2	
	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)
Treatment	0.210***	0.205***	0.214***	0.206***	0.114***	0.106***	0.147***	0.136***	0.0620***	0.0579***	0.141***	0.128***
	(0.0283)	(0.0288)	(0.0244)	(0.0249)	(0.0183)	(0.0184)	(0.0217)	(0.0210)	(0.0159)	(0.0164)	(0.0216)	(0.0217)
Women		-0.0448*		-0.0285		-0.00615		-0.0335		0.0171		-0.0126
		(0.0261)		(0.0269)		(0.0156)		(0.0236)		(0.0141)		(0.0240)
Age		-0.00262		-0.0194***		-0.0138***		-0.0222***		-0.0143***		-0.0217***
		(0.00640)		(0.00620)		(0.00475)		(0.00565)		(0.00392)		(0.00517)
Foreign		-0.0793**		-0.00980		-0.0426*		-0.0544*		-0.0323		-0.0677*
		(0.0340)		(0.0396)		(0.0252)		(0.0312)		(0.0199)		(0.0365)
Education - compulsory		0.0313		0.0488*		0.0131		0.0440*		0.000846		0.0489**
		(0.0259)		(0.0264)		(0.0174)		(0.0265)		(0.0151)		(0.0222)
Education - upper sec		0.144***		0.194***		0.117***		0.129***		0.0700***		0.151***
		(0.0354)		(0.0364)		(0.0279)		(0.0307)		(0.0259)		(0.0348)
Education - university		0.267***		0.344***		0.104		0.219***		0.0700		0.159**
		(0.0823)		(0.0742)		(0.0751)		(0.0663)		(0.0671)		(0.0793)
Work experience		0.0841***		0.0187		0.0685***		0.0455		0.0637***		0.0734***
		(0.0303)		(0.0331)		(0.0254)		(0.0280)		(0.0211)		(0.0253)
<5hrs driving experience		0.0551		0.0487		0.0548**		0.0902***		0.0588**		0.0870***
		(0.0342)		(0.0317)		(0.0271)		(0.0281)		(0.0278)		(0.0288)
>5hrs driving experience		0.194***		0.120***		0.217***		0.251***		0.156***		0.212***
		(0.0352)		(0.0392)		(0.0278)		(0.0255)		(0.0259)		(0.0268)
Moped		0.0137		0.0655		0.127***		0.0740*		0.0853***		0.117***
		(0.0414)		(0.0432)		(0.0308)		(0.0415)		(0.0295)		(0.0383)
ASR or BSR		0.00285		-0.00510		0.00225		0.0476		0.0261		0.0131
		(0.0318)		(0.0306)		(0.0229)		(0.0299)		(0.0210)		(0.0294)
CIVIS		-0.00773		-0.000389		-0.0687***		-0.0715**		-0.0525**		-0.0355
		(0.0242)		(0.0240)		(0.0242)		(0.0282)		(0.0232)		(0.0267)
Resources		-1.05e-05		-4.34e-07		-1.91e-05		-7.12e-06		-3.98e-05		7.20e-06
		(4.13e-05)		(3.94e-05)		(3.25e-05)		(3.48e-05)		(2.58e-05)		(3.36e-05)
Unemployed		-0.0409		-0.00350		-0.0304		-0.0557*		0.00182		0.00768
		(0.0341)		(0.0372)		(0.0289)		(0.0288)		(0.0238)		(0.0317)
Employed		-0.0262		0.0115		-0.0601		-0.0550		-0.00634		0.0188
		(0.0466)		(0.0461)		(0.0394)		(0.0403)		(0.0374)		(0.0412)
Independent housing		-0.0210		0.0498*		0.00548		0.0230		0.0284		0.0346
		(0.0285)		(0.0294)		(0.0233)		(0.0265)		(0.0188)		(0.0292)
Other housing		-0.0280		0.00839		0.0620**		-0.0309		0.0599**		-0.00794
		(0.0354)		(0.0375)		(0.0275)		(0.0342)		(0.0246)		(0.0364)
Population density		-3.47e-05***		-2.65e-05**		-4.54e-05***		-5.42e-05***		-3.61e-05***		-5.59e-05***
		(1.21e-05)		(1.15e-05)		(1.03e-05)		(1.10e-05)		(7.52e-06)		(1.03e-05)
Pop density squared		1.69e-09		6.97e-10		2.89e-09***		2.94e-09***		2.12e-09***		3.02e-09***
		(1.11e-09)		(8.32e-10)		(9.28e-10)		(9.92e-10)		(5.42e-10)		(9.06e-10)
Road density		0.0221		0.0463		-0.0538*		-0.0388		-0.0393*		-0.0210
		(0.0250)		(0.0281)		(0.0279)		(0.0287)		(0.0233)		(0.0254)
Railway density		-0.568		-0.952*		0.921*		0.569		0.708		0.237
		(0.457)		(0.529)		(0.523)		(0.530)		(0.433)		(0.467)
Constant	0.242***	0.229	0.398***	0.796***	0.137***	0.467***	0.296***	0.867***	0.0995***	0.390***	0.256***	0.672***
	(0.0174)	(0.168)	(0.0187)	(0.156)	(0.0186)	(0.126)	(0.0293)	(0.162)	(0.0159)	(0.102)	(0.0249)	(0.140)
Number of observations	1,561	1,561	1,626	1,626	1,821	1,821	1,893	1,893	1,821	1,821	1,893	1,893
R^2	0.048	0.114	0.041	0.095	0.020	0.144	0.020	0.148	0.008	0.106	0.019	0.138

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 4. Conditional probabilities

	12 month effect (survey 1)					24 months effect (survey 2)				
	Test	Control	Difference	Std. dev.	*p*-value	Test	Control	Difference	Std. dev.	*p*-value
Passed theory test	53.00%	34.18%	18.82%	8.20	0.0000	66.06%	47.22%	18.84%	8.01	0.0000
... first time	65.09%	62.19%	2.90%	0.82	0.4117	62.05%	62.80%	-0.75%	0.23	0.8200
Passed practical test	47.64%	40.07%	7.57%	2.07	0.0392	67.90%	63.39%	4.51%	1.41	0.1577
... first time	62.70%	58.41%	4.29%	0.78	0.4377	57.04%	53.51%	3.53%	0.84	0.4024
Total number of driving hours	24.36	24.63	-0.27	0.17	0.8687	32.21	24.63	7.58	3.71	0.0003

Table 5. Estimation results for housing outcomes

	Independent housing				Moved in last 12 months			
	Survey 1		Survey 2		Survey 1		Survey 2	
	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)
Treatment	0.127***	0.141***	-0.0509***	-0.0547***	-0.0804***	-0.0701***	-0.0363*	-0.0373*
	(0.0217)	(0.0169)	(0.0188)	(0.0186)	(0.0186)	(0.0176)	(0.0204)	(0.0200)
Women		0.0516**		0.0718***		0.000868		0.0565***
		(0.0226)		(0.0205)		(0.0182)		(0.0207)
Age		0.0174***		0.00167		-0.0116**		-0.00471
		(0.00537)		(0.00544)		(0.00495)		(0.00603)
Foreign		-0.0861**		-0.00574		-0.00696		-0.0209
		(0.0396)		(0.0327)		(0.0383)		(0.0346)
Education - compulsory		0.0490*		-0.00271		0.0636***		-0.00727
		(0.0270)		(0.0239)		(0.0197)		(0.0233)
Education - upper sec		0.0304		0.0307		0.0429		0.0291
		(0.0375)		(0.0312)		(0.0307)		(0.0322)
Education - university		0.0994		-0.113*		0.110		-0.0816
		(0.0721)		(0.0617)		(0.0690)		(0.0615)
Work experience		0.0114		0.0544**		0.0532***		0.0412*
		(0.0295)		(0.0209)		(0.0197)		(0.0231)
<5hrs driving experience		-0.00843		0.0148		0.00650		0.0112
		(0.0288)		(0.0281)		(0.0269)		(0.0300)
>5hrs driving experience		0.00773		0.0516**		0.0374		0.0185
		(0.0247)		(0.0254)		(0.0229)		(0.0271)
Moped		0.0342		-0.0555*		-0.00621		-0.0590
		(0.0324)		(0.0326)		(0.0298)		(0.0364)
ASR or BSR		-0.0171		0.00282		-0.0128		0.0277
		(0.0262)		(0.0253)		(0.0272)		(0.0277)
CIVIS		-0.0114		0.00596		0.00167		0.00326
		(0.0238)		(0.0203)		(0.0216)		(0.0229)
Resources		1.97e-05		4.31e-05		-2.98e-05		-2.40e-05
		(4.01e-05)		(4.09e-05)		(3.25e-05)		(3.99e-05)
Unemployed		0.00498		-0.0210		0.0196		-0.0437
		(0.0329)		(0.0300)		(0.0280)		(0.0322)
Employed		0.0523		-0.00718		0.0393		-0.0250
		(0.0434)		(0.0435)		(0.0361)		(0.0448)
Independent housing		0.487***		0.151***		0.183***		0.176***
		(0.0256)		(0.0279)		(0.0277)		(0.0313)
Other housing		0.138***		0.306***		0.239***		0.320***
		(0.0389)		(0.0342)		(0.0375)		(0.0344)
Population density		-3.01e-05***		-1.44e-05**		-5.93e-06		-1.39e-05**
		(1.00e-05)		(6.85e-06)		(8.52e-06)		(7.02e-06)
Pop density squared		1.59e-09*		3.99e-10		4.28e-10		2.92e-10
		(9.54e-10)		(5.95e-10)		(7.40e-10)		(5.70e-10)
% Vacant housing		2.15e-05		3.37e-07		-1.79e-05		1.43e-05
		(1.61e-05)		(1.74e-05)		(1.86e-05)		(1.85e-05)
New households		6.12e-06		9.93e-06		-1.43e-05		3.51e-06
		(2.04e-05)		(1.73e-05)		(1.73e-05)		(1.77e-05)
Constant	0.425***	-0.0806	0.300***	0.113	0.238***	0.358***	0.331***	0.326**
	(0.0189)	(0.129)	(0.0176)	(0.130)	(0.0155)	(0.124)	(0.0189)	(0.150)
Number of observations	1,821	1,821	1,893	1,893	1,821	1,821	1,893	1,893
R^2	0.016	0.240	0.003	0.090	0.010	0.077	0.001	0.074

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 6. Estimation results for employment outcomes

	Employed				Permanent job				Temporary job			
	Survey 1		Survey 2		Survey 1		Survey 2		Survey 1		Survey 2	
	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)
Treatment	-0.0148	-0.0148	0.0167	0.0136	-0.0146	-0.0145	-0.00467	-0.00797	-0.0192	-0.0196	0.0308*	0.0324*
	(0.0221)	(0.0223)	(0.0255)	(0.0249)	(0.0140)	(0.0138)	(0.0150)	(0.0150)	(0.0189)	(0.0196)	(0.0170)	(0.0170)
Women		-0.00390		-0.0368		0.00381		0.00831		0.0275		0.0116
		(0.0248)		(0.0225)		(0.0119)		(0.0157)		(0.0197)		(0.0207)
Age		0.0118*		-0.00107		0.00234		-0.000726		0.0114***		0.00188
		(0.00650)		(0.00506)		(0.00362)		(0.00406)		(0.00435)		(0.00422)
Foreign		-0.00112		0.0462		-0.00973		-0.00582		-0.00451		0.0332
		(0.0380)		(0.0329)		(0.0272)		(0.0256)		(0.0267)		(0.0274)
Education - compulsory		0.0541**		0.0814***		0.0211		0.0233		0.0310*		0.0520***
		(0.0230)		(0.0245)		(0.0160)		(0.0160)		(0.0182)		(0.0169)
Education - upper sec		0.0703**		0.129***		0.00680		0.0297		0.0409		0.0769***
		(0.0323)		(0.0358)		(0.0179)		(0.0250)		(0.0257)		(0.0275)
Education - university		0.0441		0.197**		-0.0743**		-0.0514		0.0925		0.224***
		(0.0873)		(0.0823)		(0.0306)		(0.0528)		(0.0692)		(0.0754)
Work experience		0.0326		0.108***		0.0377***		0.0345**		-0.00764		0.0382*
		(0.0253)		(0.0262)		(0.0129)		(0.0166)		(0.0224)		(0.0216)
<5hrs driving experience		0.0661**		-0.00896		-4.27e-05		-0.00746		0.0469*		0.00984
		(0.0323)		(0.0301)		(0.0183)		(0.0223)		(0.0240)		(0.0251)
>5hrs driving experience		0.0219		-0.0186		0.0120		-0.000579		0.0103		-0.0213
		(0.0281)		(0.0279)		(0.0181)		(0.0197)		(0.0214)		(0.0205)
Moped		0.0365		0.0697*		0.0146		0.0464		0.0300		0.0117
		(0.0342)		(0.0370)		(0.0228)		(0.0281)		(0.0307)		(0.0251)
ASR or BSR		0.0155		-0.0711***		-0.0173		-0.0456**		0.0214		-0.0296
		(0.0297)		(0.0250)		(0.0172)		(0.0186)		(0.0221)		(0.0204)
CIVIS		0.000814		0.00630		0.00160		-0.0340*		0.0152		0.0280
		(0.0224)		(0.0235)		(0.0129)		(0.0178)		(0.0198)		(0.0175)
Resources		-2.32e-05		2.98e-05		-3.00e-05		1.15e-05		1.25e-05		-7.70e-06
		(4.46e-05)		(4.28e-05)		(2.17e-05)		(2.76e-05)		(3.31e-05)		(3.33e-05)
Unemployed		0.0132		0.0276		0.00711		0.00987		-0.000172		0.0212
		(0.0345)		(0.0356)		(0.0199)		(0.0247)		(0.0245)		(0.0244)
Employed		0.206***		0.141***		0.112***		0.0757**		0.0878**		0.0746**
		(0.0499)		(0.0451)		(0.0289)		(0.0302)		(0.0380)		(0.0362)
Independent housing		0.0250		0.0391		0.0547***		0.0363*		-0.0334		0.000389
		(0.0293)		(0.0330)		(0.0183)		(0.0214)		(0.0219)		(0.0259)
Other housing		0.0517		0.0445		0.0333		0.0507*		-0.00241		-0.0202
		(0.0352)		(0.0368)		(0.0209)		(0.0266)		(0.0290)		(0.0277)
Population density		1.97e-05**		7.01e-06		1.53e-05**		1.45e-05***		8.28e-06		-1.53e-06
		(9.97e-06)		(6.84e-06)		(6.65e-06)		(5.16e-06)		(8.06e-06)		(5.60e-06)
Pop density squared		-1.83e-09**		-3.52e-10		-9.55e-10*		-7.50e-10**		-9.98e-10		0
		(8.04e-10)		(5.52e-10)		(5.50e-10)		(3.48e-10)		(6.55e-10)		(3.69e-10)
% Unemployment		-8.96e-06		-2.69e-05		4.27e-07		-8.76e-06		-2.79e-06		-3.33e-05*
		(2.56e-05)		(2.18e-05)		(1.42e-05)		(1.41e-05)		(1.76e-05)		(1.87e-05)
Active/employed		-5.14e-07		-6.46e-07		9.66e-06		-6.31e-08		-2.91e-06		1.66e-05
		(1.93e-05)		(1.92e-05)		(1.25e-05)		(1.25e-05)		(1.41e-05)		(1.35e-05)
Active/establishment		-5.35e-06		-1.34e-05		3.22e-06		-1.23e-06		-2.12e-05		-1.07e-05
		(1.94e-05)		(1.92e-05)		(1.23e-05)		(1.11e-05)		(1.31e-05)		(1.54e-05)
Constant	0.311***	-0.0880	0.326***	0.189	0.0959***	-0.0647	0.128***	0.072	0.163***	-0.136	0.129***	-0.00113
	(0.0150)	(0.151)	(0.0191)	(0.117)	(0.0109)	(0.0784)	(0.0127)	(0.0901)	(0.0130)	(0.0986)	(0.0130)	(0.0989)
Number of observations	1,821	1,821	1,893	1,893	1,821	1,821	1,893	1,893	1,821	1,821	1,893	1,893
R^2	0.000	0.042	0.000	0.044	0.001	0.039	0.000	0.021	0.001	0.024	0.002	0.026

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 7. Estimation results for driving outcomes; Spillovers on the non-treated

	Theory test		Practical test		Car ownership	
	Survey 1	Survey 2	Survey 1	Survey 2	Survey 1	Survey 2
Treatment	0.246***	0.224***	0.128***	0.192***	0.0905***	0.175***
	(0.0451)	(0.0404)	(0.0312)	(0.0343)	(0.0275)	(0.0387)
Saturation	0.0272	-0.123	0.122	0.179	0.153	0.118
	(0.0990)	(0.0961)	(0.125)	(0.170)	(0.111)	(0.156)
Saturation * Treatment	-0.187	-0.0897	-0.0857	-0.225**	-0.128	-0.191
	(0.161)	(0.150)	(0.0999)	(0.104)	(0.0902)	(0.129)
Constant	0.221	0.795***	0.429***	0.826***	0.345***	0.648***
	(0.172)	(0.155)	(0.119)	(0.151)	(0.0977)	(0.137)
Number of observations	1,561	1,626	1,821	1,893	1,821	1,893
R^2	0.117	0.095	0.151	0.148	0.113	0.138

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 8. Estimation results for housing outcomes; Spillovers on the non-treated

	Independent housing		Moved in last 12 months	
	Survey 1	Survey 2	Survey 1	Survey 2
Treatment	0.106***	-0.0706**	-0.0885***	-0.0811**
	(0.0306)	(0.0311)	(0.0320)	(0.0333)
Saturation	-0.166*	-0.0899	-0.0479	-0.131
	(0.0842)	(0.0765)	(0.0867)	(0.0865)
Saturation * Treatment	0.141	0.0603	0.0761	0.177*
	(0.0983)	(0.0864)	(0.0846)	(0.0939)
Constant	-0.0233	0.143	0.369***	0.357**
	(0.133)	(0.135)	(0.123)	(0.154)
Number of observations	1,821	1,893	1,821	1,893
R^2	0.241	0.090	0.077	0.075

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 9. Estimation results for employment outcomes; Spillovers on the non-treated

	Employed		**Permanent job**		**Temporary job**	
	Survey 1	Survey 2	Survey 1	Survey 2	Survey 1	Survey 2
Treatment	0.0215	0.0594	-0.00610	0.0188	-0.0104	0.0350
	(0.0367)	(0.0376)	(0.0226)	(0.0239)	(0.0328)	(0.0287)
Saturation	0.0669	0.0737	0.0328	0.0903	0.0175	-0.0428
	(0.0742)	(0.0826)	(0.0440)	(0.0573)	(0.0611)	(0.0765)
Saturation * Treatment	-0.152	-0.190*	-0.0335	-0.107*	-0.0384	-0.0152
	(0.112)	(0.150)	(0.0710)	(0.0626)	(0.0950)	(0.0965)
Constant	-0.100	0.225*	-0.0840	0.0493	-0.144	0.0470
	(0.158)	(0.122)	(0.0857)	(0.0908)	(0.101)	(0.104)
Number of observations	1,821	1,893	1,821	1,893	1,821	1,893
R^2	0.043	0.045	1,821	0.030	0.024	0.027

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 10. Estimation results for driving outcomes; restricted sample

	Theory test				Practical test				Car ownership			
	Survey 1		Survey 2		Survey 1		Survey 2		Survey 1		Survey 2	
	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)
Treatment	0.190***	0.176***	0.201***	0.192***	0.0937***	0.0747***	0.143***	0.148***	0.0377	0.0254	0.150***	0.147***
	(0.0427)	(0.0438)	(0.0362)	(0.0381)	(0.0256)	(0.0252)	(0.0315)	(0.0299)	(0.0230)	(0.0254)	(0.0325)	(0.0311)
Women		-0.0432		-0.0101		-0.00561		-0.0260		0.0326		-0.0408
		(0.0431)		(0.0448)		(0.0264)		(0.0391)		(0.0240)		(0.0420)
Age		0.00884		-0.0232***		-0.0153*		-0.0214***		-0.0138**		-0.0206***
		(0.0109)		(0.00800)		(0.00796)		(0.00740)		(0.00649)		(0.00767)
Foreign		-0.0408		-0.0240		0.00588		-0.0787*		0.0275		0.0805
		(0.0579)		(0.0676)		(0.0443)		(0.0402)		(0.0357)		(0.0561)
< bac		0.0457		0.0431		0.0317		0.0457		0.0133		0.0426
		(0.0349)		(0.0401)		(0.0255)		(0.0439)		(0.0244)		(0.0385)
bac		0.123**		0.188***		0.131***		0.100*		0.104***		0.108*
		(0.0593)		(0.0578)		(0.0373)		(0.0522)		(0.0357)		(0.0547)
> bac		0.104		0.335***		0.0506		0.348***		0.0299		0.307***
		(0.140)		(0.110)		(0.114)		(0.104)		(0.119)		(0.116)
Education - compulsory		0.0337		-0.00144		0.0621		0.0272		0.0816**		0.0618
		(0.0463)		(0.0474)		(0.0449)		(0.0401)		(0.0380)		(0.0397)
Education - upper sec		0.000830		0.0575		0.0613		0.127***		0.0900*		0.164***
		(0.0523)		(0.0499)		(0.0439)		(0.0407)		(0.0455)		(0.0410)
Education - university		0.201***		0.208***		0.229***		0.233***		0.154***		0.230***
		(0.0582)		(0.0652)		(0.0468)		(0.0423)		(0.0420)		(0.0462)
Moped		0.0681		0.0805		0.140***		0.0232		0.115**		0.0787
		(0.0743)		(0.0641)		(0.0458)		(0.0588)		(0.0469)		(0.0522)
ASR or BSR		-0.0469		-0.0213		-0.00408		0.100**		0.0122		0.0481
		(0.0462)		(0.0423)		(0.0309)		(0.0424)		(0.0293)		(0.0424)
CIVIS		0.0168		0.0108		-0.0712**		-0.0688**		-0.0614**		-0.0308
		(0.0390)		(0.0346)		(0.0295)		(0.0330)		(0.0298)		(0.0316)
Resources		9.93e-05		4.62e-05		-3.44e-05		2.39e-05		-4.38e-05		3.61e-05
		(6.22e-05)		(6.10e-05)		(5.25e-05)		(5.66e-05)		(3.76e-05)		(5.29e-05)
Unemployed		-0.0593		-0.0261		-0.0215		-0.0262		0.00765		0.0140
		(0.0608)		(0.0620)		(0.0439)		(0.0359)		(0.0375)		(0.0329)
Employed		-0.141		-0.0342		-0.0515		-0.0217		-0.00748		-0.0169
		(0.0872)		(0.0797)		(0.0547)		(0.0624)		(0.0559)		(0.0501)
Independent housing		-0.0137		0.0723		0.00912		0.0461		0.0270		0.0187
		(0.0436)		(0.0434)		(0.0338)		(0.0319)		(0.0277)		(0.0385)
Other housing		-0.0829		-0.0572		0.0740*		-0.0849*		0.0655*		-0.0669
		(0.0589)		(0.0526)		(0.0443)		(0.0505)		(0.0370)		(0.0478)
Population density		-3.41e-05*		-4.13e-05**		-3.96e-05***		-5.70e-05***		-2.90e-05**		-5.77e-05***
		(1.97e-05)		(2.03e-05)		(1.33e-05)		(1.83e-05)		(1.20e-05)		(1.64e-05)
Pop density squared		1.38e-09		7.76e-10		2.58e-09**		3.75e-09**		1.28e-09		3.50e-09**
		(1.94e-09)		(2.03e-09)		(1.25e-09)		(1.73e-09)		(1.16e-09)		(1.56e-09)
Road density		0.0553		0.0450		-0.0947**		-0.126**		-0.0554		-0.0630
		(0.0439)		(0.0481)		(0.0435)		(0.0486)		(0.0341)		(0.0420)
Railway density		-1.018		-0.586		1.697**		2.224**		1.053		1.010
		(0.782)		(0.869)		(0.807)		(0.950)		(0.653)		(0.778)
Occupational criteria		0.0196		-0.0398		0.00592		-0.0300		-0.0508		0.0192
		(0.0584)		(0.0479)		(0.0477)		(0.0486)		(0.0434)		(0.0520)
Social/financial criteria		0.0230		-0.0119		0.0825*		0.0192		0.0601		0.0563
		(0.0673)		(0.0546)		(0.0487)		(0.0435)		(0.0524)		(0.0525)
Geographic criteria		0.00622		0.0543		-0.00642		-0.0231		-0.0452		0.000300
		(0.0867)		(0.0709)		(0.0527)		(0.0787)		(0.0508)		(0.0654)
Theory test only		0.0336		-0.00385		0.103		0.0619		0.0293		0.0594
		(0.0712)		(0.0821)		(0.0722)		(0.0815)		(0.0795)		(0.0805)
Complete intensive		0.0499		-0.0628		0.00898		-0.102**		-0.0352		-0.117***
		(0.0346)		(0.0407)		(0.0525)		(0.0454)		(0.0416)		(0.0400)
Complete not intensive		0.0259		0.0495		0.0481		0.0818		0.00149		0.0490
		(0.0559)		(0.0498)		(0.0360)		(0.0509)		(0.0499)		(0.0394)
Constant	0.255***	0.0323	0.405***	0.784***	0.176***	0.536***	0.329***	0.748***	0.139***	0.357**	0.286***	0.540***
	(0.0232)	(0.291)	(0.0283)	(0.259)	(0.0303)	(0.199)	(0.0498)	(0.212)	(0.0261)	(0.158)	(0.0417)	(0.199)
Number of observations	681	681	731	731	834	834	903	903	834	834	903	903
R^2	0.040	0.105	0.037	0.121	0.013	0.165	0.019	0.187	0.003	0.132	0.022	0.178

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 11. Estimation results for housing outcomes; restricted sample

	Independent housing				Moved in last 12 months			
	Survey 1		Survey 2		Survey 1		Survey 2	
	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)
Treatment	0.102***	0.135***	-0.0753***	-0.0790***	-0.0585**	-0.0386*	-0.0380	-0.0397
	(0.0314)	(0.0238)	(0.0269)	(0.0270)	(0.0261)	(0.0226)	(0.0297)	(0.0314)
Women		0.0718**		0.0468		0.0492*		0.0503
		(0.0325)		(0.0307)		(0.0276)		(0.0314)
Age		0.0181**		0.00591		-0.0218***		0.00313
		(0.00794)		(0.00857)		(0.00763)		(0.00961)
Foreign		-0.0801		-0.0336		-0.0402		-0.0148
		(0.0663)		(0.0576)		(0.0620)		(0.0563)
Education - compulsory		0.0774**		-0.0543*		0.0863***		-0.0639*
		(0.0344)		(0.0318)		(0.0296)		(0.0327)
Education - upper sec		0.107**		0.0240		0.118**		0.0184
		(0.0498)		(0.0392)		(0.0463)		(0.0448)
Education - university		0.0473		-0.198**		0.221**		-0.121
		(0.101)		(0.0966)		(0.104)		(0.0953)
Work experience		0.0467		0.0504*		0.0836***		0.0253
		(0.0471)		(0.0298)		(0.0313)		(0.0317)
<5hrs driving experience		-0.0396		0.0305		0.00899		0.0258
		(0.0478)		(0.0467)		(0.0391)		(0.0508)
>5hrs driving experience		0.0163		-0.00393		0.0389		-0.0526
		(0.0410)		(0.0313)		(0.0324)		(0.0343)
Moped		0.0212		-0.0303		0.00878		-0.0559
		(0.0494)		(0.0506)		(0.0471)		(0.0521)
ASR or BSR		-0.00907		0.00786		-0.0488		0.0532
		(0.0361)		(0.0365)		(0.0347)		(0.0436)
CIVIS		-0.0314		-0.0214		0.0153		-0.0502
		(0.0349)		(0.0287)		(0.0376)		(0.0319)
Resources		4.18e-05		7.37e-05		1.35e-05		-1.53e-05
		(6.17e-05)		(6.20e-05)		(6.28e-05)		(6.41e-05)
Unemployed		0.0318		0.00497		0.0599		-0.0446
		(0.0488)		(0.0424)		(0.0444)		(0.0465)
Employed		0.0262		-0.0523		0.0396		-0.0489
		(0.0660)		(0.0546)		(0.0570)		(0.0637)
Independent housing		0.488***		0.155***		0.173***		0.187***
		(0.0416)		(0.0391)		(0.0401)		(0.0501)
Other housing		0.197***		0.323***		0.313***		0.279***
		(0.0627)		(0.0524)		(0.0538)		(0.0505)
Population density		-2.04e-05		1.20e-05		-2.04e-05		6.16e-06
		(1.44e-05)		(1.46e-05)		(1.53e-05)		(1.58e-05)
Pop density squared		-2.94e-10		-3.19e-09**		1.98e-09		-1.71e-09
		(1.44e-09)		(1.41e-09)		(1.46e-09)		(1.46e-09)
% Vacant housing		-9.21e-06		-1.91e-05		-3.64e-05		1.69e-06
		(2.52e-05)		(2.65e-05)		(2.81e-05)		(3.14e-05)
New households		-6.94e-06		-7.26e-06		-4.35e-05		-9.29e-06
		(2.62e-05)		(2.80e-05)		(2.88e-05)		(2.84e-05)
Occupational criteria		0.0743		-0.00737		-0.0469		0.0188
		(0.0462)		(0.0399)		(0.0465)		(0.0336)
Social/financial criteria		0.0130		-0.0214		0.00589		-0.00657
		(0.0434)		(0.0382)		(0.0536)		(0.0399)
Geographic criteria		0.107**		-0.0331		-0.00748		-0.0476
		(0.0426)		(0.0379)		(0.0441)		(0.0350)
Theory test only		0.0600		-0.0424		-0.107		-0.00151
		(0.0696)		(0.0559)		(0.0734)		(0.0593)
Complete intensive		0.0349		-0.0370		0.0610		-0.125***
		(0.0389)		(0.0376)		(0.0404)		(0.0397)
Complete not intensive		-0.0507		0.0721**		-0.0498		0.0574*
		(0.0334)		(0.0315)		(0.0319)		(0.0334)
Constant	0.456***	-0.133	0.326***	0.101	0.239***	0.645***	0.348***	0.166
	(0.0275)	(0.168)	(0.0254)	(0.220)	(0.0226)	(0.188)	(0.0251)	(0.243)
Number of observations	834	834	903	903	834	834	903	903
R^2	0.010	0.269	0.007	0.108	0.005	0.121	0.002	0.080

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10%

Table 12. Estimation results for employment outcomes; restricted sample

	Employed				Permanent job				Temporary job			
	Survey 1		Survey 2		Survey 1		Survey 2		Survey 1		Survey 2	
	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)	OLS (1)	OLS (2)
Treatment	-0.0261	-0.0289	0.0399	0.0288	-0.0275	-0.0255	0.0128	0.00516	-0.0191	-0.0216	0.0234	0.0153
	(0.0369)	(0.0364)	(0.0419)	(0.0397)	(0.0223)	(0.0205)	(0.0216)	(0.0218)	(0.0293)	(0.0312)	(0.0258)	(0.0240)
Women		0.0117		-0.0420		-0.000160		0.0148		0.0373		-0.00332
		(0.0310)		(0.0374)		(0.0217)		(0.0243)		(0.0228)		(0.0333)
Age		0.00417		-0.00547		0.00107		-0.00520		0.00369		0.00579
		(0.00894)		(0.00818)		(0.00587)		(0.00630)		(0.00691)		(0.00582)
Foreign		0.0301		0.0406		0.00509		0.00973		0.0104		0.0385
		(0.0679)		(0.0521)		(0.0397)		(0.0338)		(0.0433)		(0.0451)
Education - compulsory		0.0548		0.0825**		0.0294		0.0368*		0.0536**		0.0492*
		(0.0366)		(0.0382)		(0.0229)		(0.0220)		(0.0264)		(0.0282)
Education - upper sec		0.115**		0.107**		0.0215		0.0432		0.100**		0.0541
		(0.0507)		(0.0446)		(0.0309)		(0.0321)		(0.0390)		(0.0372)
Education - university		-0.0753		0.141		-0.0353		-0.0232		0.0362		0.168
		(0.107)		(0.123)		(0.0736)		(0.0881)		(0.0883)		(0.115)
Work experience		0.0551		0.137***		0.0356**		0.0416		-0.00605		0.0673*
		(0.0378)		(0.0454)		(0.0174)		(0.0277)		(0.0328)		(0.0362)
<5hrs driving experience		0.0444		0.00211		0.0111		-0.0108		0.0245		0.0448
		(0.0444)		(0.0493)		(0.0269)		(0.0347)		(0.0383)		(0.0432)
>5hrs driving experience		0.0295		-0.0540		0.0132		-0.0140		0.00324		-0.0232
		(0.0395)		(0.0401)		(0.0245)		(0.0296)		(0.0311)		(0.0296)
Moped		-0.00497		0.0547		-0.000419		0.0429		0.00171		0.0168
		(0.0555)		(0.0556)		(0.0356)		(0.0383)		(0.0414)		(0.0394)
ASR or BSR		0.0695		-0.0567		0.0173		-0.0678***		0.0360		-0.0320
		(0.0432)		(0.0390)		(0.0233)		(0.0250)		(0.0346)		(0.0311)
CIVIS		-0.00640		-0.0146		-0.00212		-0.0317		0.0309		0.0100
		(0.0298)		(0.0308)		(0.0193)		(0.0244)		(0.0309)		(0.0245)
Resources		-8.38e-05		2.78e-05		-3.57e-05		5.42e-05		3.75e-05		2.52e-05
		(7.11e-05)		(5.86e-05)		(3.26e-05)		(4.97e-05)		(5.46e-05)		(5.16e-05)
Unemployed		0.00987		0.0876*		-0.0318		-0.0131		0.0255		0.0723**
		(0.0498)		(0.0515)		(0.0365)		(0.0457)		(0.0390)		(0.0312)
Employed		0.319***		0.175**		0.0832*		0.0180		0.167***		0.127**
		(0.0783)		(0.0708)		(0.0462)		(0.0530)		(0.0579)		(0.0508)
Independent housing		0.0427		0.0574		0.0636*		0.0154		-0.0287		-0.0191
		(0.0475)		(0.0540)		(0.0322)		(0.0400)		(0.0338)		(0.0430)
Other housing		0.00146		0.00167		0.0164		0.0435		-0.00248		-0.0447
		(0.0536)		(0.0505)		(0.0310)		(0.0386)		(0.0437)		(0.0415)
Population density		7.74e-06		8.07e-06		1.31e-05		1.75e-05		-3.46e-06		1.52e-06
		(1.85e-05)		(1.88e-05)		(1.14e-05)		(1.16e-05)		(1.49e-05)		(1.14e-05)
Pop density squared		-8.97e-10		-0		-8.47e-10		-1.20e-09		2.39e-10		-5.14e-10
		(1.84e-09)		(2.11e-09)		(1.11e-09)		(1.27e-09)		(1.35e-09)		(1.33e-09)
% Unemployment		-2.73e-05		-1.40e-06		1.99e-05		1.32e-05		-2.52e-05		-2.97e-05
		(4.14e-05)		(3.48e-05)		(2.50e-05)		(2.20e-05)		(2.90e-05)		(3.35e-05)
Active/employed		-1.47e-05		8.05e-06		-8.81e-06		-1.85e-05		4.07e-06		4.14e-05*
		(2.97e-05)		(3.31e-05)		(1.70e-05)		(2.09e-05)		(1.92e-05)		(2.26e-05)
Active/establishment		-8.14e-06		-2.62e-05		5.22e-06		5.12e-07		-8.40e-06		-1.88e-05
		(2.86e-05)		(2.57e-05)		(1.67e-05)		(1.61e-05)		(2.07e-05)		(2.18e-05)
Occupational criteria		-0.126***		-0.00685		-0.0127		0.0140		-0.0829**		0.00180
		(0.0451)		(0.0366)		(0.0261)		(0.0320)		(0.0412)		(0.0285)
Social/financial criteria		-0.00333		-0.000226		-0.0158		-0.0196		0.0116		0.0166
		(0.0474)		(0.0424)		(0.0272)		(0.0374)		(0.0459)		(0.0331)
Geographic criteria		0.0111		0.0760		0.0289		0.0248		0.00822		0.0158
		(0.0533)		(0.0564)		(0.0370)		(0.0355)		(0.0508)		(0.0350)
Theory test only		0.00398		-0.0132		0.0201		0.0257		0.0181		-0.00447
		(0.0574)		(0.0630)		(0.0397)		(0.0444)		(0.0379)		(0.0564)
Complete intensive		-0.0406		0.0301		-0.0661***		-0.01000		0.0497		0.00426
		(0.0550)		(0.0392)		(0.0227)		(0.0286)		(0.0478)		(0.0302)
Complete not intensive		-0.0859*		0.0337		-0.0696***		-0.00485		-0.0263		-0.00925
		(0.0483)		(0.0445)		(0.0255)		(0.0287)		(0.0351)		(0.0258)
Constant	0.310***	0.155	0.320***	0.271	0.101***	-0.0413	0.117***	0.169	0.159***	0.0545	0.132***	-0.110
	(0.0239)	(0.233)	(0.0285)	(0.201)	(0.0170)	(0.126)	(0.0172)	(0.136)	(0.0203)	(0.160)	(0.0182)	(0.147)
Number of observations	834	834	903	903	834	834	903	903	834	834	903	903
R^2	0.001	0.089	0.002	0.054	0.002	0.064	0.000	0.036	0.001	0.051	0.001	0.039

Notes: OLS with standard errors clustered at the local site level
*** significant at 1%, ** significant at 5%, * significant at 10

Figure 1. Geographical coverage of the 154 local sites participating in the experiment

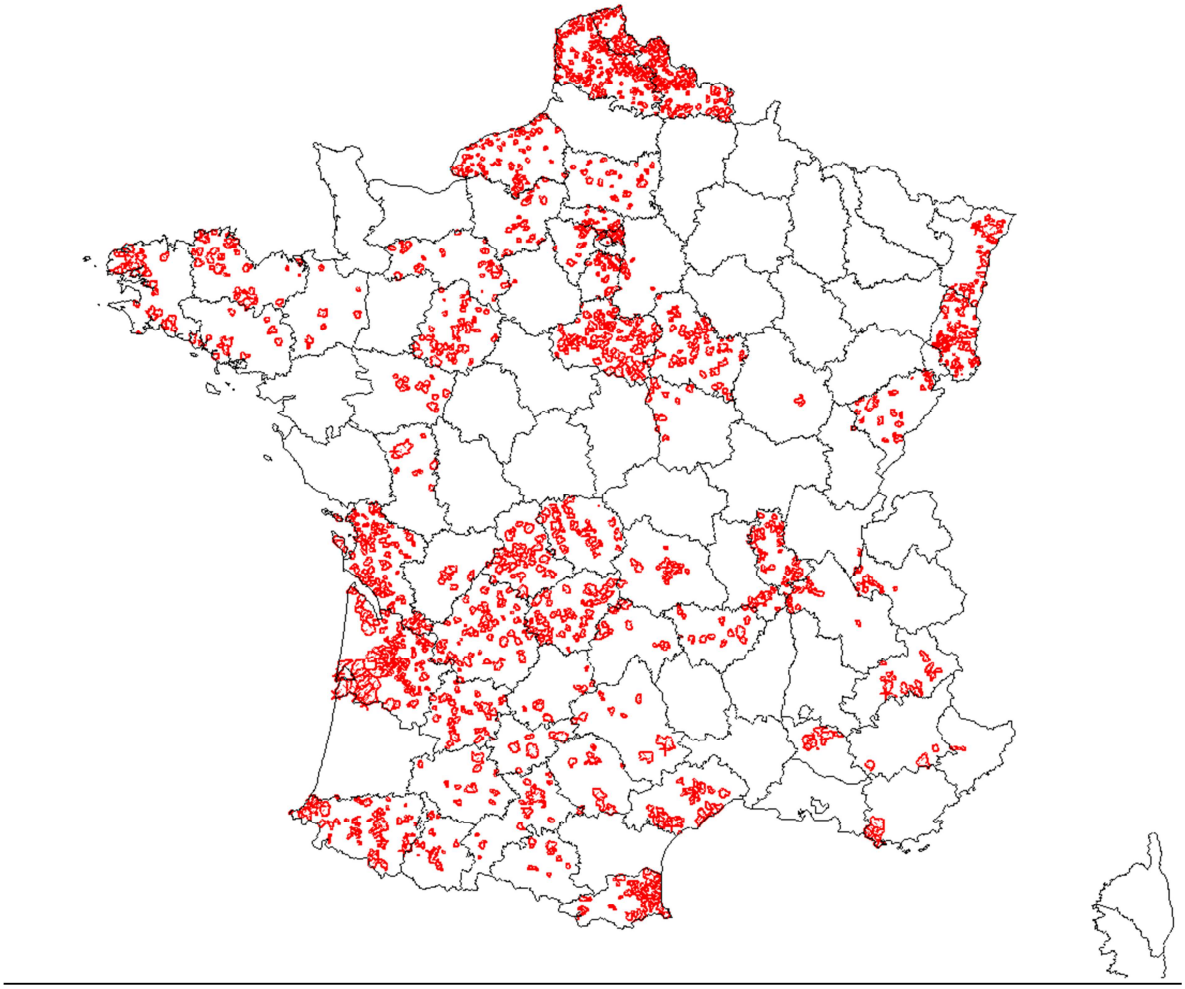


Figure 2. Calendar for the experiment

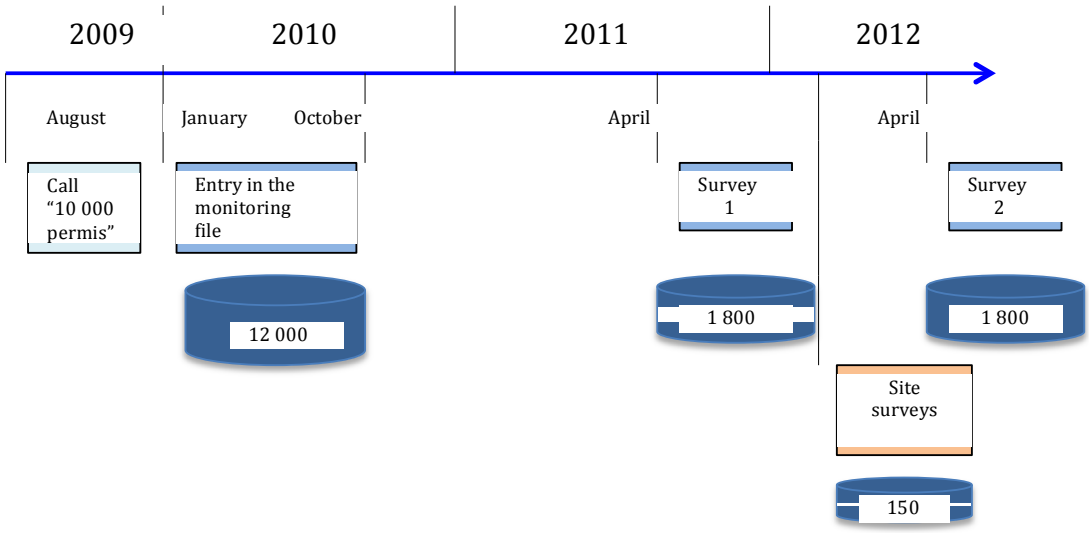


Figure 3. Evaluation protocol

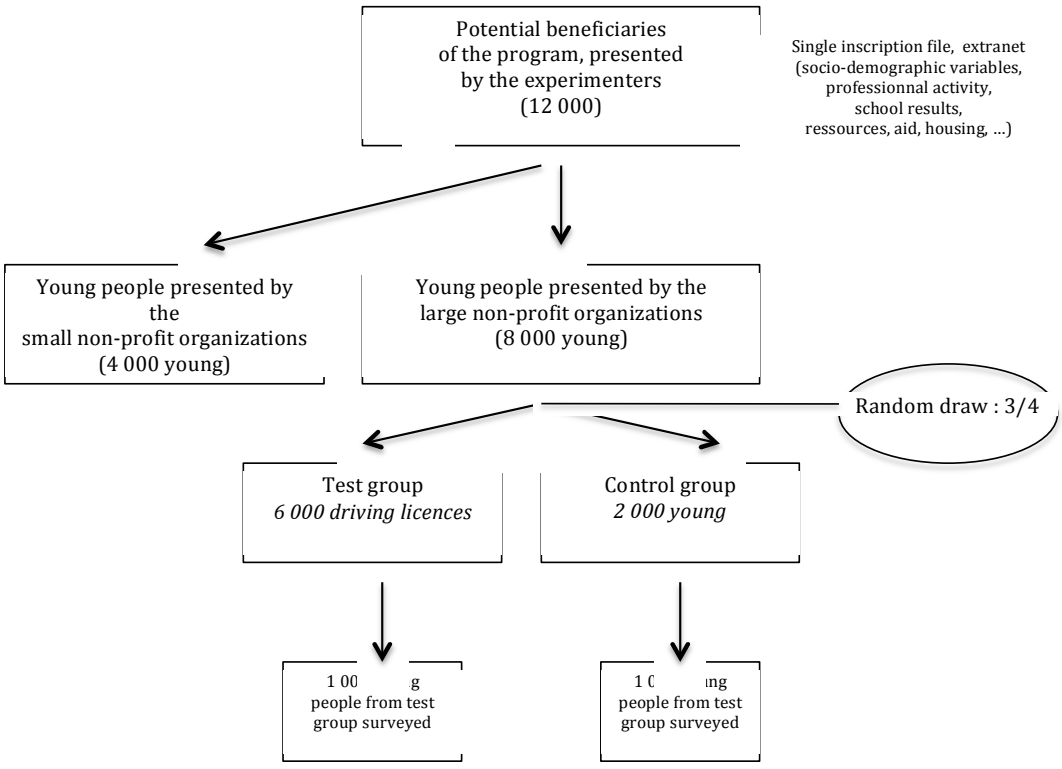
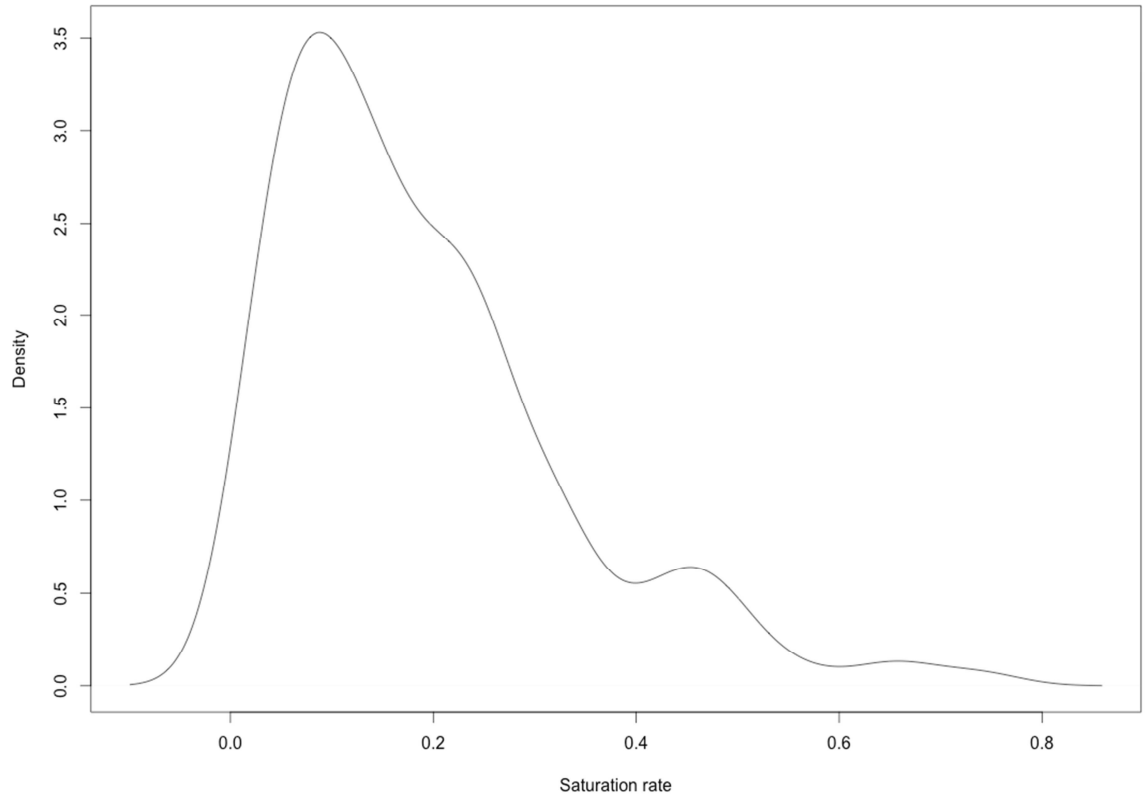


Figure 4. Density of saturation rates over the 154 local sites



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